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XXXI.—*The Relationships of the Porifera.* By Dr. G. C. J. Vosmaer. (Translated by Arthur Dendy, B.Sc., F.L.S.*)

It is now eighteen years since Oscar Schmidt remarked, "A natural system of sponges still awaits its founder;" and this is still the case. A qui la faute? When Schmidt published his first works on Sponges the subject was certainly in a worse condition than at the present day, and he was the first who earnestly endeavoured to inaugurate a better state of affairs. It is his merit rather to have foreseen than seen many natural relationships, and thereby to have laid the foundation of a natural system. We have, however, already seen in the systematic part of this work that Schmidt's system can, in short, no longer be used. The facts which we owe to more recent methods of research have thrown a somewhat different light upon these matters, and hence I believe that I was right in making several modifications in the system—modifications which I hope are, in part at any rate, at the same time improvements. I have repeatedly pointed out that the system is still far from being a natural one; but I have as much as possible taken into consideration genealogical questions. Meanwhile the linear arrangement of the group necessarily

* From Dr. G. C. J. Vosmaer's work on the Porifera in Bronn's 'Klassen und Ordnungen des Thierreichs' (1887), pp. 472–481.

adopted in the systematic portion of the work could not clearly elucidate questions of relationship, and hence the necessity for a more careful consideration of the question in this place. Many people have placed the calcareous sponges in too close connexion with the siliceous ones, largely owing to Fritz Müller's vain attempt to derive the calcareous and siliceous structures from horny fibres. I have here followed the main division of Gray, and have accepted two classes—Calcarea and Non-calcarea—a proceeding as to the correctness of which people seem to be more and more agreed. The first spongologist of the present day, F. E. Schulze, accepts this classification*. There are absolutely no transitions between the two classes; and since the spicules appear at a very early date in the larva, it can be only the very earliest developmental phases which are common to the two. This primary division thus appears to be a natural one.

The Porifera Non-calcarea appear to me to be divisible into three orders:—Hyalospongiæ, Spiculispongiæ, and Cornacuspongiæ. The Hyalospongiæ † all have this in common:—their skeleton is composed of spicules based upon the triaxonid type. The Cornacuspongiæ are distinguished by a new element, spongin; and in the Spiculispongiæ the "spicula" are the chief distinguishing feature. It appears to me that the genera within each order are more nearly related to each other than to the genera of other orders; and if this be so, as I shall immediately endeavour to show, then the classification is a natural one. These three orders nevertheless are not nearly so sharply separated from one another as are the two classes. We do not, it is true, know of any direct transitions from the Hyalospongiæ to the other orders. Still there are certain facts which perhaps indicate a possible connexion. Schulze appears to accept no connexion at all when he says, "and however plausible, indeed almost self-evident is the hypothesis that the latter (six-rayed spicules) may also atrophy and give rise to spicules with fewer axes, so that the spicules might even all become monaxonid, we know as yet no Monaxonia in whose spicules we can detect any indication (such as through crossing canals) of a descent from triaxonid spicules" ‡. But we do find in the literature statements to

* 'Ueber den Bau und das System der Hexactinelliden' (Berlin, 1886), p. 32.

† Hexactinellidæ, auctorum; but I prefer on principle to give names with similar endings to equivalent divisions, rather than to abide rigorously by priority. In the case of genera and species, on the contrary, I keep as much as possible to the laws of priority.

‡ *Loc. cit.* p. 34.

this effect, though very sparingly. I refer here to *Stylocordyla borealis* (Lov.), Wyv. Thomson, in which Lovén has actually found the rudiments of the axial cross *. And do not most of the spicules of this sponge indicate that spicules which have such a swelling are originally descended from six-rayed forms? I might further point to the peculiar spicules of *Suberites lobiceps*, O. S. † The so-called "anchor-spicules" [M. ta. $\phi < 90$] are, as is well known, very abundant in Hexactinellids; and the question arises, how far are the similar structures in the Tetraxonina related to them? The same holds true of the little chelate spicules ‡ of the Desmacidonidæ.

The possibility that all originally descended from Hexactinellid-like ancestors, and that the occasional isolated appearance of spicules with remnants of a triaxonid form may thus be attributed to atavism, is to me personally not unlikely.

This occasional appearance is much more common than people think; I have found many sponges in which there are a few rudimentary six-rayed spicules lying among the normal ones. This is somewhat analogous to the case of men with rudimentary tails or with extraordinary hairiness. And so it is with those Halichondrine forms which, in the arrangement and form of the parts of the skeleton, recall certain Suberitidæ (*Clavulina*). The common characters of all the three orders named are more numerous and more important than those of the two classes, and thus they are more intimately connected.

Coming now to consider the closer relationship of the forms within each order, no one will take exception to the Hyalospongiæ as a natural group. The mutual connexion of the Spiculispongiæ rests upon the following grounds:—The examination of the different suborders of the Spiculispongiæ appears to me to show unmistakable signs of degeneration. Leaving out of account the Lithistina, which, owing to their peculiar knotty structures, stand somewhat on a separate footing (although the condition of the canal-system and ground-substance, as well as the often well-marked tetraxonid skeletal elements, distinctly show their affinities), we may perhaps assume that the Tetraxonina represent the older forms. The presence of distinct tetraxonid spicules, a more or less distinctly radiate arrangement of the skeleton, a more or less distinctly pronounced cortex, the granular character of the ground-substance, and a rather highly developed canal-system are their characteristics. We see all this most

* I may in the meanwhile refer to my work "Sponges of the 'Willem Barents' Expedition," 1880 and 1881, pp. 10–12.

† Spong. atlant. Gebiet. p. 47, pl. v. fig. 5.

‡ $=anc.^2$ and *anc. anc.*

distinctly in the Geodidæ and in many Ancorinidæ. Amongst the Corticidæ and Plakinidæ a marked reduction in the multiplicity of the skeletal elements has taken place; the latter family also show clearly how triradiate spicules, nay even simple styli*, may arise from quadriradiate forms. In the Oligosilicina the reduction of the spicules has gone still further; *Chondrilla* retains only the characteristic euasters † or spherasters ‡. In *Chondrosia* and *Oscarella* the skeleton has completely vanished; but they have retained the characteristic granular condition of the ground-substance. *Oscarella* is nearly related to *Chondrosia*, and *Chondrosia to Chondrilla*. But now the step from *Chondrilla* to *Corticium* does not appear to be very great, and so I believe in the existence of a connexion between the so-called askeletal forms and the true Tetraxonina.

In many Tetraxonina we see a kind of tendency to lose the tetraxonid spicules, and we find more and more frequently the long, smooth, peculiarly shining, radially disposed styli coming to the fore. But the Tethyadæ are forms in which this degeneration has become complete. The arrangement of the smooth shining styli is, however, still markedly radiate; fibres and stellate spicules are still present and also the granular ground-substance. Finally, these same conditions in the Polymastidæ mark a transition to the Suberitidæ.

Turning now to the connexions of the Cornacuspongiæ *inter se*, we find here the newly acquired spongin attaining the first importance, while the spicules ultimately completely vanish. Many *Halichondriæ* still show a resemblance to the Suberitidæ; but the arrangement of the skeleton is always more irregular, *i. e.* less distinctly radiate; and with this fact must be connected the gradual loss of definite external form. It seems to be generally agreed that there is really a close relationship between the Halichondrina and the Ceratina; indeed most of the younger spongologists have repeatedly brought forward new arguments in favour of this view. I refer particularly to the works of von Lendenfeld § and myself ‖. I am therefore somewhat surprised that Schulze should seem inclined to lay so much stress upon the entire absence of siliceous spicules.

We have thus considered somewhat more closely the relationships of the Sponges, and the question now arises, How can one represent to one's self their connexion, *i. e.* their descent? From what has been said, every one may judge for himself

* = *Stabnadeln*, Vosm.; *acuates*, Bk.
† "Sternchen." ‡ "Kugelsternchen."
§ 'Zoologische Anzeiger,' 1884, no. 164.
‖ Mitth. zool. Stat. Neapel, Bd. v. 1884, p. 490.

whether what follows is purely hypothetical, and how far it is so. What is the ancestral form from which the Sponges have been derived? This question has been answered in a variety of ways; but all the answers are hypothetical, for our embryological knowledge is too limited and imperfect. It appears to me that it is as yet simply impossible to say what may have been the appearance of the ancestral sponge. We have, it is true, reason to believe in the existence of a free-swimming form, which may have looked something like the larva of a siliceous sponge, but not like that of *Sycandra* or similar certainly aberrant forms.

Before Leuckart's time (1854) the Sponges were regarded as undoubted Protozoa. But when their complex structure gradually became known, and especially after Huxley's statements concerning the presence of ova and spermatozoa in *Tethya*, Leuckart first expressed the opinion that the Sponges belong to the Cœlenterata; and, indeed, up to a short time ago this was the generally accepted hypothesis; until at length the third possibility was perceived, namely that they might occupy a separate position between the two. This view has again found an advocate in Heider's latest work. In 1880 I indicated it in my Inaugural Dissertation. Balfour * is of opinion that they form an "independent stock" of Metazoa, and Sollas also. There can scarcely be any doubt that the Sponges are not Protozoa. It is also certain that there are, on the other hand, important differences between true Cœlenterata and Sponges. Even those investigators who enthusiastically maintain the Cœlenterate nature of the Porifera place them as a natural, separate group, in opposition to the Cnidaria. We are not, however, dealing only with the question, Are the Porifera a subtype of the Cœlenterata or a special type? but also with the phylogenetic reasons. Although the Sponges may not be Protozoa, yet they may have descended from Protozoa. If we can hold in general that the Metazoa are descended from Protozoa, and if we further admit that Sponges are true Metazoa, then forthwith we stand face to face with the question, What are the phylogenetic relations of the Sponges to the remaining Metazoa? With regard to this the results arrived at by Sollas and Bütschli essentially agree. Bütschli maintains "that the Sponges form a group which is completely separated from the remaining Metazoa and which originated from the Choanoflagellata (Saville Kent) quite independently." Independently of Bütschli, Sollas came to the same conclusion; he

* 'Comparative Embryology,' i. p. 122.

names the "Phyllum," separately descended from the Protozoa, *Parazoa*, the remainder *Metazoa*. Marshall now stepped forward in opposition, endeavouring further to support the opinion which he had previously expressed. He first said * :—"Porifera and Telifera (*sit venia verbo*) are two divergent branches of the Cœlenterate stock, which have arisen from the common stem-form of the Protactinia." And he now † adds to this :—"It may readily be granted that the ancestors of the Sponges had not yet for very long, perhaps never at all, possessed tentacles, which, however, are something secondary; but they were at least two-layered, and, besides, as we may conclude from the occasionally forthcoming cases of reversion, radiate; they had a mouth-opening and a gastral cavity, from which gastral canals came off centrifugally, and, breaking through the ectoderm, opened freely outwards; and such creatures are, according to my understanding, under all circumstances true Cœlenterates." Schulze ‡ criticizes the views of Bütschli, Marshall, and the older authors, and himself comes to the conclusion that very probably the oldest sponges possessed no radial evaginations of their central cavity, but were, like the *Olynthus* amongst the Calcarea, simply sac-shaped.

Let us now examine these two views, which so strongly contradict one another. I will begin with Marshall's theory, as it is the most definitely formulated. It rests mainly, as the author himself allows, on the radiate structure, which, however, according to him, the Sponges have lost. He views Sponges as degenerate animals, and indeed degenerate Cœlenterates, a view which Dohrn, ten years before, and also Balfour § had already put forward as possible. Balfour is, however, very doubtful :—"It might perhaps be possible to regard Sponges as degraded descendants of some Actinozoon type, such as *Alcyonium*, with branched prolongations of the gastric cavity; but there does not appear to me to be sufficient evidence for doing so at present. I should rather prefer to regard them as an independent stock of the Metazoa." I believe every one who has engaged in spongological researches is often struck with the idea of degeneration, but cannot always bring this into harmony with other things. And hence, perhaps, Balfour's doubt. It appears to me that people have always regarded the question too generally, and, on the other hand, too one-sidedly, and have not thought of the

* Zeitschr. für wiss. Zool. Bd. xxxvii. p. 246.
† Jen. Zeitschr. Bd. xviii.
‡ Sitzber. Akad. Berlin, 1885.
§ 'Comparative Embryology,' i. p. 122.

possibility that what is true for one division of the Sponges is certainly false for another. It seems to me, and everything points to this conclusion, that most siliceous sponges are degenerating in a certain respect, but that in the Cornacuspongiæ a new force has stepped in which again lifts them up, and that the Calcarea of the present day are also developing progressively. But even if most Sponges do show numerous traces of degeneration, yet they need not on that account be descended from Cœlenterates. The differences between the two groups are so great that even the most zealous advocate of their cœlenterate nature, as we have seen, puts their phylogenetic connexion a very long way back; and, in spite of this, Marshall's theory is scarcely tenable. Granted that the nearest ancestors of the Sponges were "at least two-layered," granted also that they were "radiate," even that they possessed a "gastral cavity" (*s. l.*), &c., yet this shows nothing. Such creatures are still not Cœlenterates. Marshall, to be sure, goes further, and claims for the sponge-ancestor a "mouth-opening" and a "gastral cavity" with centrifugal canals; but there are no grounds for this. For, as Heider again asserts, the so-called osculum of the Sponges is neither homologous nor analogous with the mouth of Cœlenterates, and the large internal cavity present in many Porifera has just as little claim to the significance of a gastral cavity as, in short, the canals in connection with it have to be placed on the same footing as the peripheral canals of the Cœlenterata. There is not a single reason for regarding the central cavity in Sponges as a gastral cavity. Even supposing that its epithelium may, perhaps, take up nutrient particles, still it has never yet been observed that the cavity is the true digestive cavity, *κατ' ἐξοχήν* *. This is, moreover, very improbable for several reasons; for, in the first place, this momentous cavity is not always present, or it is very small; and, in the second place, its position and arrangement are very unfavourable for the retention of solid bodies. It may be answered, that it has not yet been demonstrated that proper solid nutriment is taken in. Since, however, it is certain that particular sponge-cells *can* take in solid bodies, and do so very readily, and, further, that sponges placed in reservoirs which are kept as clean as possible, and where the inflowing water is freed from suspended particles, perish more rapidly than others which are kept in dirty (*sit venia verbo*) reservoirs, it is, on this ground alone, more probable that solid nutriment is a vital question with them. The unfavourable nature of the position of the so-called gastral cavity depends

* Häckel's assertions rest upon pure imagination.

often (much oftener than at first one is inclined to believe) upon the fact that the large aperture faces downwards, and upon the relatively powerful current, the so-called gastral cavity being the common canal, the cloaca, into which all the other canals open. And even in cases where spicules project into the "stomach," which might eventually retain nutriment, these spicules are constantly curved towards the osculum so as to prevent entrance, but in no way preventing exit.

Their developmental history teaches us that the Porifera and Cœlenterata separate from one another at a very early date. As Heider correctly and expressly insists, the Sponge-gastrula attaches itself by the mouth, while the Cœlenterate-gastrula attaches itself by the aboral pole. Thus the two types proceed together as far as the gastrula-stage, but then each goes its own way. Finally, Balfour * has already pointed out the early appearance and great development of the mesoblast as a striking difference between Porifera and Cœlenterata. Thus if I cannot agree with those who would regard the Sponges as Cœlenterates, I also do not agree that they have descended from Cœlenterates.

In considering the question whether the Sponges have descended from Protozoa we must, in order to avoid misunderstanding, distinguish between a direct descent (*i. e.* regarding the question as Saville Kent does, and then, as a necessary consequence, viewing the Porifera as a progressively developing group) and an indirect descent (supposing Sponges in general or sponge-ancestors to have been derived as Metazoa from Protozoon colonies). The latter view appears to me the most plausible. We can hardly imagine a direct descent. I will not further urge the conclusion that Sponges are not colonies of Monads or Choanoflagellata; but the differences between the Sponges of the present day and the Protozoa are also so great that we can only properly discuss the question whether the ancestors of Sponges descended from Protozoa; and in this sense I can only answer the question in the affirmative, it being still left quite uncertain in what manner the transition was brought about.

It is well known that Balfour started with the Amphiblastula larva, and saw therein the ontogenetic recapitulation of a parent-form which stood between Protozoa and Metazoa. He assumes that the cells of the two halves differentiated themselves functionally into *nutritive* (the amœboid cells) and *respiratory-locomotive* (the flagellate cells). When the sponge became attached these (locomotive) flagellate cells must for the most part have become functionless, while the amœboid cells,

* 'Comparative Embryology,' ii. p. 285.

being of great use to the whole colony, increased. Hence arose a larger external layer of nutritive cells and a small internal layer of now chiefly respiratory cells.

This theory of Balfour's is criticized in Heider's latest work, and the arguments brought forward certainly seem to us very powerful. "Balfour," says our author, "was wrong in summarily dismissing the question whether we have not perhaps in the amphiblastula-larva a cenogenetically modified form." As such Heider considers it, especially as the amphiblastula is present only in the Calcarea, and not in all of these.

Secondly, Heider thinks that we have yet no right to regard the amœboid cells as more proper to the reception of nutriment than the flagellate cells. He points here to the Salpingœcæ and Codosigæ, and maintains that our knowledge of the mechanism of the motion of the flagellum is too slight to enable us to form an opinion as to the powers of the collared cells. In the third place, he objects that Balfour does not explain why the larva should have given up free movement.

Heider now puts forward another hypothesis, based upon his recent researches on *Oscarella*, wherein he assumes "that the cavity of invagination is the gastral cavity, and that the cells of the invaginated layer, thus in *Sycon* the flagellate cells, were originally the nutrient elements." The gastrula-like parent-form of the Sponges then gave up its free-swimming mode of life "because it placed its mouth against the surface of some solid body, in order in this manner to seek food on the surfaces of stones swarming with minute organisms of all sorts." The attachment took place originally in the manner which Heider discovered in *Oscarella*, *i. e.* only at single points, so that water could flow into the gastral cavity all the time. There is certainly much to be said for this hypothesis; but if Heider objects that Balfour does not explain why the ancestral form becomes attached and gives up its free-swimming habit, we may, on the other hand, object that Heider does not say why the blastula-like larva ever turns into a gastrula. What was the *principium movens* in this case? Everything appears to me to be still pure hypothesis, to which one can only oppose other hypotheses. I will willingly grant the possibility that the Metazoa may have been derived from colonies of Protozoa. This is very probable, but not necessary; but so long as we do not yet know which cells of the sponge and of the sponge-larva are nutritive * and which subserve respiration, so long will it be of

* Polejaeff considers it to be tolerably well shown that the collared cells are very badly adapted to taking in food, and he supports this hypothesis chiefly on mechanical grounds. It must not be forgotten, however, that as yet we know scarcely anything of micro-mechanics.

little avail to seek to explain how a sponge-larva or a primitive sponge has arisen from a colony of Protozoa. Balfour's theory is based upon pure assumption, and so is Heider's. It would be just as possible that, after the functional differentiation had taken place in the cells of a colony of Protozoa, the larva became, owing to the formation of spicules, too heavy to swim and sank to the ground, wherein lies a great incentive to become attached. The early, often very early, appearance of the spicules may be urged in favour of this view. But all this, as we have said, is as yet pure hypothesis, for which certainly much may be adduced; but it appears to me still rather purposeless to philosophize much about the matter.

If we accept a free-swimming form as the ancestor, and suppose further that solid structures became secreted in certain cells (thereby conferring an advantage in rendering these delicate forms of life less subject to fall a prey to other animals), then we must at the same time believe that in one group calcareous and in another siliceous matter was developed. But this new development led to the restriction, nay finally even to the complete prevention, of free movement, and thereby a higher animal development was precluded. Sessile animals must develop in a special direction in order to maintain the struggle for existence. Nutrition and respiration must be assured; hence, though the degree of development is a low one, yet a well-developed canal-system has been formed.

A second supposition to which we are forced is that Sponges originally lived in tolerably great depths. The oldest forms are, emphatically, deep-sea forms. When, at a later date, they also lived in shallow regions, we see in arrested development the consequences of such a proceeding. The whole class of *Porifera non-calcarea* appears to indicate this. First the skeleton degenerates, the relative amount of silica decreases, and the variety of spicular forms is step by step reduced. At the same time the independent characteristic form is lost; but in certain examples the canal-system develops progressively, not in constant, although probably direct, or inverse relation to the skeletal system.

Thus from the primitive form have arisen, in the first place, the Calcareous Sponges, a group in which the canal-system is most complex in those forms which show degeneration in the skeleton. From the primitive form of the Calcarea, perhaps an *Olynthus*-like sponge, arose, on the one hand, the *Asconidæ*, and, on the other, the ancestors of the Sycons, from which the *Syconidæ* of the present day have been developed; but also, as we have fairly good reasons for believing from Poléjaeff's researches, the *Leuconidæ* and *Teichonidæ*. The position of the *Pharetronidæ* remains doubtful.

In the second place, from the primitive form have been developed the Siliceous Sponges, and certainly forms with triaxonid spicules. From these arose first the fossil and recent *Hyalospongiæ*, then, by the disappearance of the proper triaxonid spicules and the formation of tetraxonid spicules, the *Tetraxonina*. The stock which gave off the lateral branch *Hyalospongiæ* produced later on the branches *Lithistina*, *Geodidæ*, and *Ancorinidæ*. From the *Ancorinidæ* arose the *Plakinidæ* and *Corticidæ*, and doubtless also the *Chondrosidæ* and *Halisarcidæ*. One portion, however, gave off the branch *Tethyadæ*, then the *Polymastidæ* and *Suberitidæ*, while the main stem, always degenerating, ran out into the *Halichondridæ*. The newly acquired spongin developed more and more and made the spicules superfluous; thus arose progressively the *Spongidæ*, *Aplysinidæ*, and *Darwinellidæ*.

As already said, I wish to make no definite assertions concerning the main stem, but only to give a possible picture of the ramification of the most important branches.

There has also been much dispute about the question of the germinal layers. Schulze, after several vacillations, has finally expressed himself very decidedly:—"In addition to the collared cells of the flagellated chambers the whole of the single-layered and continuous epithelium, composed of pavement-cells, lining all the cavities, passages, and canals of the exhalant system, from the exhalant openings of the flagellated chambers to the margin of the oscular opening, is formed from the *endoderm*" *. On the other hand, "the layer of flattened epithelium which clothes the outer surface of the sponge and all the inhalant fissures and canals, from the free surface to the inhalant pores of the flagellated chambers, is formed from the *ectoderm*" †. The remainder of the body is derived from the mesoderm. Schulze certainly seems to wish to extend these remarks, in the first instance applied to *Plakina*, to the entire group of Sponges. According to Marshall ‡ the larva (of *Reniera filigrana*) consists of an "ektoderm" and "cœnoblast," which later on divides into "entoderm" and "mesoderm." From this "entoderm" arises the entire canal-system, while the "ektoderm" furnishes only the epithelium which clothes the outside of the body. The third very different view is that of Gœtte. According to him § the larval ectoderm vanishes, and consequently the entire sponge is formed from endoderm.

* Zeitschr. für wiss. Zool. Bd. xxxiv. 1880, p. 438. † *Loc. cit.*

‡ Zeitschr. für wiss. Zool. Bd. xxxvii. 1882, pp. 221–246.

§ 'Abhandlungen zur Entwickelungsgeschichte der Thiere,' iii. Untersuchungen zur Entwickelungsgeschichte von *Spongilla fluviatilis*.

In the presence of such contradictory opinions, all of which, without exception, have very slight foundation in fact, it certainly seems best at present to keep silence. According to most authors the "endoderm" and "ectoderm," whatever may be their distribution in the body, furnish only the epithelia. All the rest—genital products, skeletal system, in general the body proper—is formed from "mesoderm." Every spongologist will doubtless, then, be somewhat startled to learn from Kleinenberg * that there is generally no mesoderm present.

We may shortly sum up our results in the following sentences:—

1. The Sponges must not be classed amongst the Cœlenterata. They form a type of their own.
2. The Sponges are probably descended from free-swimming forms, which, originally without supporting structures, ultimately developed a strong skeleton.
3. These primitive forms lived at great depths.
4. Coincidently with life at less depths degeneration of the (siliceous) skeleton took place.

XXXII.—*A Reply to Dr. G. J. Hinde's Communication "On the Genus* Hindia, *Dunc., and the Name of its Typical Species."* By Prof. P. Martin Duncan.

After a careful study of Dr. Hinde's paper (Ann. & Mag. Nat. Hist. Jan. 1887, p. 67) I find that it adds very little to our previous knowledge of the interesting Silurian sponge. It is important that the geographical range of the form should have been increased, and it is exceedingly satisfactory that Dr. Hinde should have been able to find some siliceous spicules the shape of which corroborates the statement made by me that the form resembled a tetraclade lithistid. The bulk of the paper consists of criticisms, partly self-contradictory, however, and unsatisfactory in their tone, and partly useful in re-exposing possible errors which had already been discovered by Dr. Rauff.

Dr. Hinde endeavours to explain the strong contradiction regarding the value of Rœmer's specific diagnosis by asserting that the casts described by that author are recognizable as the casts of the species *H. fibrosa*=*H. sphæroidalis*, nob.

* Zeitschr. für wiss. Zool. Bd. xliv.